

Envelope-to: mlr1000@cus.cam.ac.uk
Delivery-date: Fri, 13 Feb 1998 06:36:20 +0000
Date: Thu, 12 Feb 1998 23:35:42 -0700 (MST)
From: ALWYN VANDERMERWE <avanderm@du.edu>
Subject: Re: Ridderbos and Redhead. REPORT 2AR
X-Sender: avanderm@odin.cair.du.edu
To: "m. l. redhead" <mlr1000@cam.ac.uk>
MIME-version: 1.0
X-UIDL: 15309ad20eed302472a972cca4d6e1c9

Dear Authors: Please respond to the report 2AR below. Many thanks.
Cordially, AVDM.

REPORT 2AR

Dear Prof. van der Merwe,

I do not want to stand in the way of publishing this paper especially if there have been other positive referee reports and it is the wish of the journal to do so. In general I think that the topic is interesting and perhaps publishing it will further work on the problems the authors address.

Having said this, I must confess that I'm not convinced by the authors' responses to my report. Let me make a few remarks about the authors' reply.

1. It still isn't clear to me that haven't begged several questions in their rejection of Sklar's line on the spin echo results. They say, of the coarse-graining approach, that its advocates will have to appeal to the history of the system---a move that they (the coarse-grainers) do not have to make in the case of other systems. Isn't it the case that the interventionist must also explain this behavior? In the paper they say that "the kind of thermodynamic behaviour we would like to explain using statistical mechanics is the behaviour which leads to the usual situation in which an innocent observer unaware of the history of the system will actually make the *right* prediction, namely that the syustem is going to stay in the equilibrium state for all future times." Is it the authors' claim that statistical mechanics should not be expected to play a role in the explanation of the innocent's surprise? How does the interventionist explain the fact that the innocent makes the *wrong* prediction? Or, again is that not something that can be/should be explainable by the interventionist?

As I've said, I may very well be missing something here that is important, but it does seem to me as if restricting SM explanations to "true equilibration" begs the question against Sklar who seem to think that the observed anti-thermodynamic behavior is something SM can and should play a role in explaining.

2. Looking back at the paper I see that the authors don't assert explicitly that it is a virtue that their model has no stronger properties than mixing, though it stills seems that the context of the remark is naturally read in that way. The rejection of ergodic approaches as candidates for explanation (because of KAM type results), however, seems to me to be premature. These results may very well play a crucial explanatory role even if the system may be slightly nonergodic.

3. Finally, I agree that there is an important difference between restricting ones explananda to systems which are small subsystems of larger

systems/environments and the limitations due to features about observational accuracy. On the other hand, I think I may be excused from the criticism of discussing them in the same context given this sentence of the authors' paper: "In the interventionist account, the emphasis is shifted from a limited measurement resolution towards measurements which are restricted to limited, interacting systems."

My main worry here was and is that there is a fairly standard line against interventionist approaches which the authors seem to ignore; namely, that one can always "expand the 'system'" to include part of the larger environment. Then the interventionist line with respect to the original "system" won't seem to apply (or so the argument goes). In effect this is why I asked what justifies the delimitation of the system as the 'system' to be investigated.

Envelope-to: mlr1000@cus.cam.ac.uk
Delivery-date: Tue, 23 Dec 1997 19:55:40 +0000
Date: Tue, 23 Dec 1997 19:55:39 +0000 (GMT)
From: "T.M. Ridderbos" <tmr23@cus.cam.ac.uk>
Reply-To: "T.M. Ridderbos" <tmr23@cus.cam.ac.uk>
To: "Prof. M.L. Redhead" <mlr1000@cus.cam.ac.uk>,
"Dr J.N. Butterfield" <jb56@cus.cam.ac.uk>
Subject: new address
MIME-Version: 1.0
X-UIDL: f59d1f54ae23b8fd1471bcc3a8555b07

Dear Michael and Jeremy,

I will leave Cambridge on 24th December. My new address will be:

Van Hogendorpstraat 101-4

1051 BL Amsterdam

The Netherlands

tel: + 31 20 6822583

(I will continue using my Cambridge e-mail address: tmr23@cus.cam.ac.uk)

I will be back in Cambridge during 13-15 January, because of the Darwin interview; I will keep you informed about the jrf developments.

Merry Christmas and all the best for the new year!

katinka

Principle B

Effectively the system is "exporting" its correlations to the environment, but of ~~course~~^{the}, the argument can be repeated for the environment of the environment, so the larger system consisting of the original system with its ~~environment~~^{which} and its intermediate environment, will also ~~stuff~~^{export} or receive or send general activity due to perturbations from the "environment of the environment"